

TIMELINESS OF REFEREEING

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Abstract

This note offers a few thoughts arising from the reading of Stein and Meng (1995), a report on a panel discussion held at the 1995 Orlando A.S.A. national meeting on the topic "Speeding the Referee Process."

Being an Associate Editor of *Biometrics* in the 1960s showed me the shameful games *sometimes* played by editors, authors and referees. For example, the author told to reduce his paper's length apparently did so by retyping with narrower margins and many displayed equations incorporated into text; and the editor who acknowledged holding a paper for three months before sending it to a referee; and a referee who within a week returned a 30-page paper with a three-word comment, "Very good; publish." The second referee of the same paper never responded to any correspondence whatever, including never refereeing the paper.

Fortunately such heinous behavior is infrequent. Nevertheless, referees in our profession are infamous for being too slow in doing their work. Two examples are a recent 4-page article that was still unrefereed after seven months; and currently awaited are referees' reports on an 8-page manuscript seven months after submission!! Even worse is Larry Brown's noting in Stein and Meng (1995) "that many *Annals* referees believe that six months to a year is an acceptable length of time for producing a referee's report." To my mind that is an attitude that should be considered as totally unacceptable as professional behavior. Indeed it is disgraceful. Surely we must put a stop to this attitude. Why? and How? I offer some suggestions — if not acceptable, at least they might promote discussion, and perhaps ultimately help bring about a changed outlook.

There are many reasons why we should change the current procrastinatory behavior of referees. Consider just three.

- (a) Scientific journals in being, as they are, just replacements of personal correspondence, are vehicles for dissemination of new knowledge. And new knowledge deserves to be disseminated while it is still new. Editorial and printing delays are bad enough, but additional months of delay caused by referees should not be tolerated. That delay represents selfishness to the author and to ultimate readers.

(b) Publication delays are incentive to the “underworld” of pre-prints, which are usually available to only a select audience. That seems eminently unfair to other ultimate readers.

(c) When a paper gets buried on a referee’s desk and becomes unearthed months after arrival, might not there be bias in the resulting report? Hopefully the referee feels guilty about the delay, which could well lead to a hurried reading of the paper and an ineffective critique; contrarywise that guilt could just as likely produce heavy-handed refereeing.

For answering the “How?” question at least three ideas come to mind:

(1) **Pay Referees**, with a premium for promptness (an additional 25%, say, as was recently suggested by a book publisher). Unfortunately, most journals could not afford to pay referees. But maybe a variation of this could work: allow authors to provide funds to the journal for paying referees!! What a hornet’s nest that would be! But this highlights an inherent difficulty: referees get no reward to-day, and so editors have no stick with which to beat recalcitrant referees.

(2) **Change the requirements expected of a referee.** In Stein & Meng (1995), Paul Switzer and Rob Kass are reported as raising this issue; and reference is made to Gleser (1986) specifically saying that “line by line checking is not expected by referees.” Stein and Meng comment that “panel members agreed that what was of greatest importance to them as editors was an assessment of the contribution a paper made, assuming the results were all true.” I heartily agree. This seems to be the heart of the matter; and in this regard Gleser’s (1986) article says it all. Whilst the desire of having referees check mathematical details is admirable, it has been made impractical by the pressure of limited publication space: most journals prefer papers of say, no more than 10-15 printed pages. This severely curtails publication of intermediate mathematical steps, which leads to proliferation of phrases such as “it can be shown that...”. Under these circumstances it seems unfair to require referees to slog through (i.e. reconstruct) mathematical details that are not in the paper. This is akin to asking referees of papers in the experimental sciences (e.g., nutrition, chemistry, physics, medicine, and so on) to re-do experiments as part of the refereeing process.

So why expect this in statistics? “ To make sure a paper is correct” say our inner souls. Yes, but we know that there is no 100% guarantee of this — and if striving for it delays publication by 6-12 months is it worthwhile? Let the burden of correctness fall where it should: on authors. After all, we all use computing packages that have not been (cannot be) checked 100% and which periodically throw out an error (the pentium chip?) and no one expects referees to check these packages.

(3) **Change statisticians' culture toward refereeing.** The “culture of slowness” was, as reported by Stein and Meng (1995), “noted by many panelists as the main obstacle to speeding up the referee process.” This is where we need to put our effort, to CHANGE THIS CULTURE. As part of our professional ethos every single one of us should accept and live by two precepts:

- I. We must accept a few refereeing jobs each year.
- II. Acceptance of a refereeing job implies that that job becomes top priority in one's work schedule — priority to the point that the referee's report will *without fail* be in the mail within 4 weeks of receiving the paper.

Surely it should not be difficult for the profession to live by such a dictum. Of course, in I above the question will arise as to what is “a few” jobs. That is entirely up to each would-be referee. No one has to accept every refereeing request; and so long as non-acceptance means *immediate* return of the paper there can be no complaint. Item II above is what will be considered impossible. That is where pressure of every kind must be brought to bear: conference sessions, reminders in journals, persistent hassling by editors, whatever it takes to bring a change. Combined with everyone's right to not accept every refereeing request received, along with responsibility to abide by I, bolstered by changing the requirements expected by a referee [as in (2) above] so that refereeing is less onerous than now, I see no reason why our “culture of slowness” cannot be abandoned. It is something of which statisticians should be very ashamed.

References

- Gleser, L.J. (1986) Some notes on refereeing. *The American Statistician*, **40**, 310-312.
- Stein, M. and Meng, X-L. (1995) Speeding up the Referee Process, *The Institute of Mathematical Statistics Bulletin*, **24** (6), 607-608.